

mencement of this notice, that the example set by the chief and staff of the West Riding Asylum is one worthy to be followed by the officers of all similar institutions. There are probably few establishments in this country that afford such opportunities as the one which gives its name to the volume before us, but there are still fewer, if indeed there are any, that cannot contribute something new and of scientific and practical value to neurological medicine. One institution in this country, the Utica Asylum, furnishes a quota in the *American Journal of Insanity*, published and edited by its accomplished superintendent, Dr. Gray, and his able corps of assistants. In other places, also, with less means, careful scientific investigations have been taken up and successfully prosecuted, as, for example, by Dr. Kempster and his assistants at the Wisconsin Northern Asylum at Oshkosh. But as a rule we think American and foreign alienists have confined themselves largely to the practical duties of their positions, and their scientific contributions have been few and far between. We do not say this in reproach, for we know that the burden of executive responsibilities placed upon asylum superintendents and physicians often precludes any attention to the less directly practical pursuits. We only hope that in the future it may be that with wider reading and knowledge on the part of the physicians, and more generous support on the part of the public, the magnificent opportunities afforded for the study of the pathology of the nervous system by these establishments may be further utilized for the benefit of mankind.

---

IV.—LECTURES ON DISEASES OF THE NERVOUS SYSTEM. BY JEROME K. BAUDUY, M. D., &c., &c. Reported by V. Biart, M. D. Revised and Edited by the Author. Philadelphia. J. B. Lippincott & Co. 1876. Pages 484.

Useful as this modest work is likely to prove to the general practitioner, we must express regret that it should have been published, until by wider reading and maturer reflection, it could have been brought nearer the present standard—none too high—of scientific and literary excellence. The feeling that the author is capable of better things, and will live to justify this feeling, induces us frankly to record this judgment.

The plan,—general make-up, of the book,—and we will add its spirit, are good, but it bears the marks on almost every page of haste, of a want of fine discrimination, of loose analysis, and of a lack of acquaintance with the valuable results of recent

Continental research. In spite of these drawbacks, we repeat, the work has very considerable practical merit, and we have had a degree of pleasure and profit in its perusal.

It consists of forty lectures, delivered during the winter session of 1874-'5, in the Missouri Medical College, and which the author informs us he was induced to publish by the request of the alumni of the school. Dr. Bauduy frankly acknowledges his indebtedness to a list of writers, which he names, and formally disclaims for himself in his work, originality "either as to its facts or its theories." His reading seems to have been confined to English authors, and to the productions of the few French and German writers whose works have been put into an English dress. But such material as he has availed himself of, seems to have been industriously utilized.

In the present notice it will be impossible for us to review Dr. Bauduy's book at length. We have already and in this present number, noticed at length another work, treating much the same subjects: viz, the work of Dr. Hammond.

In speaking of "hyperæmias of the Brain," Dr. Bauduy says, "In congestion the temperature is not elevated." But this is often not true, especially for active hyperæmias. Again we find him saying: "One marked characteristic feature of the symptoms of cerebral congestion, is, that they are *general* and *diffused*, not *localized* or *limited*." But if the reader will turn to our notice of the corresponding part of the work of Dr. Hammond, certain statements will be found suggesting serious doubts as to the accuracy of Dr. Bauduy's declaration. We believe to the contrary, that *localized* or *regional* congestions, are more common than the "general and diffused."

But on the whole this and the succeeding chapter on "General Cerebral Anæmia," though entirely devoid of novelty, will be found safe guides to the student and general practitioner. As respects cerebro-spinal meningitis, Dr. Bauduy says: "I do not think it a disease of the nervous system, any more than I think typhoid fever is to be assigned to such classification." He says elsewhere it "should be grouped with the essential fevers." But we can go no further at present than to say that it would not be difficult to show the contrary opinion to be more nearly correct. We hope to have an opportunity in the future of treating this subject at length. Dr. Bauduy does not place any considerable reliance on any known means for treatment, but it seems to us he underrates the value of opium and the salts of its alkaloids in this disease.

Insanity seems to be fairly treated, within small compass, the author closely following Dr. Maudsley. But there is nothing novel, either in matter or method, in the lectures on this subject. Dr. Bauduy relies much on the buckthorn, (*Rhamnus frangula*) as a cathartic in insanity. As regards the pathology of insanity, but little is said, and that little is upon the whole unsatisfactory. It is, indeed, one of the principal defects of the

work: viz., the comparative absence of careful discussions of the pathology of the affections treated. Chorea and choreic-hemiplegia, our author decides not to have a cerebral origin. He thinks the phenomena of this group of affections may be "explained by the atonic condition of the nervous system, resulting in complete neurasthenia, attributable to the prolonged disturbances." (P. 249.)

But this summing up of their pathology is provokingly vague, and does not throw any true light on their nature and mechanism. For ourselves we do not see how a careful analysis of the phenomena of chorea can lead one to any other than an intra-cranial seat, either in the basal motor ganglia, or the cortex, or both.

Says Dr. Bauduy: "Whenever there exists a paralytic condition of certain muscles, produced by a pathological state of the nervous centres, just in proportion to the want of use of these muscles will there be a corresponding atrophy, which, if not prevented or arrested, will end in a fatty degeneration of the ultimate muscular fasciculi. But in hysterical palsy this is not the case," etc. (P. 252.) But as we understand the case, neither of these propositions are strictly true. In the first place it is certainly not true that all pathological conditions of the motor tract which are capable of producing muscular paralysis, lead to atrophy and degeneration of the fibres of the paralyzed muscles, except that which results from simple disuse. As respects their relation to muscular atrophy and degeneration, pathological conditions of the motor tract must be divided into at least two classes, depending on their character, and to a certain extent on their seat: namely, those which are followed by a degree and kind of muscular lesion which cannot be ascribed to simple disuse, and those which are not; and the pathological condition (for there is one) in hysteria which leads to paralysis belongs to this latter class, and the principal seat of the morbid action, we may add, is intra-cranial.

In the second place it is not strictly true that there is no muscular atrophy in hysterical paralysis, though it is admitted in most cases to be slight, and for reasons which are intelligible and in the main satisfactory. But we cannot enter into an adequate discussion of this subject, for it is a wide one.

In the discussion of the pathology of *progressive muscular atrophy*, Dr. Bauduy says finally: "Such is precisely the clinical characteristic of progressive muscular atrophy, the anatomical and pathological condition of which is a lesion of the sympathetic or *trophic* system." And again, "in conclusion, we may affirm that the characteristic pathological and anatomical alteration in progressive muscular atrophy, is a lesion of the sympathetic nervous system." Certainly the author could not, at the time of writing, have been aware of the vast mass of information which has been brought to light in respect to the pathology of this affection the last few years. While it has been held by many writers, such as Meryon, Wachsmuth, Oppenheimer, Hasse,

Meyer, Friedberg, Malmsten, Roberts, Friedreich and others, that the muscular lesion is a primary one, independent of the nervous system, it has been held by others to the contrary, that the muscular lesion is consecutive to an affection of the related nerves, or rather nervous centres. Among those who have held to this opinion, we may mention Cruveilhier, Duchenne, Romberg, Schneevogt, Valentiner, Cohn, Frommann, Virchow, Bergman, Ollivier, Erb, Trousseau, Charcot, Joffroy, Lockhart Clarke, Hayem, Vulpien and a host of others. At this moment the weight of fact and opinion is on this latter side, and we do not see how, in view of the case as it now stands, the contrary view can be maintained. Dr. Bauduy would seem to have adopted the earlier opinion of Jaccoud on this subject, which, however, that eminent observer seems to have since changed for the one rejected by our author.

Then again the statements we have quoted would appear to imply that the sympathetic nervous system has trophic functions, for he speaks of it, as the "sympathetic or trophic system." We are wholly at a loss as to the anatomical or physiological grounds for such an opinion. We do not deny that it may exercise trophic functions, but we do deny that the state of our knowledge on the subject, is such as to warrant us to any degree, in attributing to it such functions. It is the "vaso-motor," and *not* "the trophic system." By a misprint, we presume, the mistake is made of placing the "intervertebral ganglia" on the anterior roots of the spinal nerves. (P. 314.)

In speaking of "reflex paralysis," he says by this phrase "is meant a paralysis which is not dependent for its origin upon any primary disease of the cord, but is of a reflex nature, and caused by some peripheral irritation. This occurs when irritation of the uterus, of the bladder, or of the penis, etc., *is turned back or reflected to the spinal centres producing a paralysis etc.*" But we do not wholly understand this description. That a peripheral irritation may cause a paralysis, we can understand, but not Dr. Bauduy's description of the *way* in which it is produced. He says the irritation (we presume) "*is turned back or reflected to the spinal centres etc.*" "Turned back" from whence?

If the irritation is reflected say, from the uterus, what is its primal source? In our view, the irritation (peripheral) is not, "reflected to the spinal centres," as Dr. Bauduy seems to hold, but *from* them to some other part, perhaps to the walls of certain muscular vessels. And beside the evidence afforded in this passage, there is a certain indefiniteness of view as to the mode of production of a reflex paralysis, running through the whole lecture. He appears to side with those who, like Dr. S. Weir Mitchell in this country, and Jaccoud abroad, have adopted the "exhaustion theory," instead of that which is called "reflex," which affirms in effect, that an irritation of some organ, say the uterus, is propagated along centripetal nerves, to the spinal cord, and is then reflected from closely related vaso-motor

centres, along vaso-motor nerves to the vessels of some parts of the body, say those of the cord, with the immediate effect of causing their contraction, and in this way producing a certain degree of anæmia. The anæmia is supposed to lead to functional inactivity, and hence to paralysis of those muscles which receive their motor-nerves from that part of the cord, which is the seat of the anæmia. It may be doubted whether an anæmia can be produced in this way sufficiently complete to cause paralysis. We doubt it ourselves. It has been doubted whether the prolonged vasal spasm, necessary to explain the continuance of such a paralysis—could occur. We do not doubt but that prolonged vasal spasm may exist, notwithstanding all that has been said to the contrary. But we do not have the time nor space at present in which to give our reasons for such a belief. We believe a temporary paresis rather than a paralysis, may be produced in this way, but with more certainty in the way supposed by Dr. Mitchell, Jaccoud and others. But we hold that most cases of the so-called reflex paralysis are due to actual structural change, simply nutritive, or even destructive in the cord, combined with circulatory changes, either anæmia, or—more probably—congestion.

In his lecture on progressive locomotor ataxia, Dr. Bauduy adopts the view of certain physiologists, that the "cerebellum does *not* preside over the co-ordination of muscular movements." It is true that the cerebellum is not alone concerned in this function. That the pons, medulla and cord, all share in it is probable. But the conclusion of our author seems to be, that it has no such function in any sense. This we need hardly say is a view shared by but few very competent physiologists at this hour. He adopts also the view that there is not a muscular sense. This is not only an erroneous but to us a strange doctrine, in the light of the general principles of nervous physiology.

In the discussion of locomotor ataxia, the following language is used: "The ganglionic substance in the cord, has nothing physiologically in common with the posterior columns, which are composed only of white fibres, and convey sensation, but deep in the cord is the gray matter, rich in ganglionic cells, their function is not the transmission of sensation, but the *production* of nerve force, etc.," (P. 382.) On this quotation we remark first as to the function of the fibres of the posterior columns of the cord. It is said they convey sensation, [more strictly sense-impressions] the same idea as that which has prevailed for at least half a century, and which was almost rendered classic by the authority of Longet. It is probably true, that many fibres of these columns subserve the purpose named, but it is also probably true, that many more of them, form part of the spinal apparatus of co-ordination for muscular movements. Then again we had supposed, ever since the researches of Dr. Brown-Sequard on the spinal cord were made known, almost a quarter of a cen-

tury since, that it was admitted that the gray matter has as *one* of its functions, that of transmitting sensory impressions in the cord toward the brain. But this does not seem to be the view of Dr. Bauduy. He would seem to be unaware of the most recent views in respect to the *role* of the posterior columns, in the spinal mechanism of co-ordination. But we cannot discuss this subject farther, in this place.

We have followed Dr. Bauduy's account of "diphtheritic paralysis," but have not found any satisfactory discussion of its mode of production.

In the discussion of neuralgia, we find many things to animadvert on. For example: In order to explain how it comes to pass that in neuralgia, very slight sense impressions give rise to pain, we have the following:

"To explain the occurrence of pain from an irritant which is normal in intensity and quality, may seem a difficult matter, but a simile may serve to make it plainer. You know that the tone produced by the vibration of a string of a musical instrument, depends for its pitch not only upon its size and length, but also upon the degree of tension to which it is subjected. Now you may consider a nerve in the normal degree of tension as comparable to a musical string giving the tone of C, *natural*. When the tension is increased, so that the same excitement produces the sensation of pain, the tension of the string will be such as to produce C. of the octave above. When this condition is carried to the degree that the sensation becomes intolerable, agonizing, it will correspond to the highest note produced when the string is stretched to the utmost extent: further increase in the tensile force, and the nerve is paralyzed,—the wire breaks. Again the different forms of physical force, heat, light, electricity, etc., have an influence upon the tone produced by a musical string, by raising and lowering its tension. The transmission of nervous influence may be compared to that of a vibration or undulation through a wire." (P. 445.)

We have quoted this passage for two purposes, first as an example of that common mode of explaining a neuralgia, which fixes attention solely, or nearly so, on the nerve tracts and their actual or presumed alterations, instead of those of the nerve centres with which the diseased nerve tracts have such close, anatomical and physiological connections, and secondly, as an example of a loose and erroneous use of physical analogy which is so common in dealing with biological questions. Is there any good reason for thinking that a nerve may be truly compared to a wire in different states of tension, and that different degrees of the conductivity for painful impressions may be compared to the different notes which can be evoked from the same under the different conditions named? Not the slightest. The force of an analogy lies in the completeness of the similarity of the cases. In this one the parallelism is a pure assumption, that will not bear a moment's steady examination. And the mis-

fortune of the analogy lies in the fact that it leads one away from the principal seat of morbid action—the *nervous center*—the sole true seat of sensibility. Instead of speculating vainly as to how the conductivity of a nerve fibre may be increased above the normal state so as to *magnify* sense impressions, the true way to find an explanation for the fact that slight sense impressions cause pain in neuralgia, is to recognize that by reason of irritative, nutritive disease of the sensory cells of the centres, which stand connected with the diseased nerve trunks, the sensibility to pain, or painful impressions of which they are the seat, is so much exaggerated that impressions of an intensity which are, under normal relations, agreeable, give rise to pain, when delivered in the centre. The key to the situation lies not in the nerve trunk but in the related nerve centre, which all such hypotheses as we have just quoted overlook.

This, with one or two other leading errors, vitiates the lectures of Dr. Bauduy on neuralgia from beginning to end, especially the confusion of hyperalgesia with hyperæsthesia. The former is always present in a neuralgia, the latter comparatively seldom, while in many cases there is a positive degree of anæsthesia, even during the height of the painful attack.

But here we must terminate our hasty survey of Dr. Bauduy's work. We have simply selected a few of the more important points, the discussion of which, it appeared to us, would probably prove most useful or suggestive to our readers.

The style of the work is seriously marred for the general public by the too frequent play of an ill-regulated enthusiasm, or an indulgence in the language of fulsome encomium, when speaking of certain favorite authors, and the too frequent reproduction of the harmless pleasantries of the lecture room, and of something akin to the paternal, patronizing spirit, which there is so much of a temptation to assume by the occupant of a professorial chair toward the learners gathered about him.

It is also unpleasant to meet so often with tautological phrases, such as "congestive hyperaemia," and unusual phrases, to say the least, like "sanguinolent succulence," etc.

We would not close our notice of Dr. Bauduy's work, however, without reiterating what we have said; before, that it is on the whole a useful one to a large class in the profession, but it cannot be expected to often win in the race for professional favor, by the side of the work of Dr. Hammond, or the three or four volumes on nervous disease of Ziemssens' Handbook, all of which will be shortly placed before the American medical public in an English dress.